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Psychological Monographs: General and Applied

RESIDUA OF SHOCK-TRAUMA IN THE WHITE RAT: A THREE FACTOR THEORY[1]

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IT IS no longer necessary to argue for a relationship between early experiences and later adjustive behavior and capacities. An extensive clinical and experimental literature has burgeoned in the past two decades. Initially, much of the work of experimental investigators was prompted by a desire to explore and test the ideas of clinical workers. However, by their probing, experimental investigators have established a reverse tradition; now clinical workers tend to use the results of experimental research as support and stimulation for further clinical observation and judgment (Bowlby, 1953). Such a "redress" is surely the prelude to a deeper and more penetrating union of clinical and experimental insights. In an area so rich in problems and data that have both theoretical and practical importance, one has every reason to expect and hope for an amalgamation of experimental and clinical thinking.

One of the oldest traditions of speculation about the relation between early experience and adult behavior centers on the effects of traumatic and unusual experiences suffered by infant organisms. From the Wild Boy of Aveyron, to Riesen's chimpanzee (1958) and Harlow's rhesus (1958) there runs the thread of interest in what happens when the infant organism's environment is seriously modified. In all these studies, the main interest has usually not been in the pathology itself—fascinating thought it may be—but in the information it provides about the processes and outcomes of normal development. The present report is in this tradition, dealing with the hypothesis that intense and unusual stimulation early in life may produce profound and persisting effects on later behavior.

Much of the research in this area has focused on the effects of unusually impoverished or enriched environments where treatment extends over long periods in the life of the organism. This program, however, was designed to investigate the effects of a relatively narrow range of intense stimulation within a brief period of the life of immature rat pups. More recently there has been an increase in the number of investigators also studying more limited and precisely known treatments (Ader, 1959; Denenberg & Bell, 1960). Nevertheless, there is a good deal of similarity between the objectives of the more narrowly defined research and the earlier investigations. They both are interested in the effects of different opportunities during infancy for perceptual and problem solving activities, and they both seek to discover and understand the nature

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of these experiences and the mechanisms and processes by means of which they determine later behavior.

Because there have been several thorough reviews of recent research we shall not undertake one here. The final section, however, contains a discussion of some related investigations in connection with a number of questions and problems raised by our findings. It seems fair to say, however, that all of the reviewers agree on the necessity for more careful and systematic work because the practice of postulating some infantile conditions or experience to explain a set of observations upon adults leads to an uncritical and ready acceptance of whatever favorable material appears. If the explanation of certain observations of adult behavior rests upon matters which can be directly and fairly easily studied, then such investigations should be carried out. This is surely the only way to temper "wild" appeals to early experience.

Methodological Considerations

As in other research areas there are certain unique methodological problems which confront investigators and considerable attention has been given to them by recent writers (Ader, 1959; Baron, Brookshire, & Littman, 1957; King, 1958). Although it is true that studies of early experience must contend with design problems which are often more demanding than those in other research areas—the longitudinal nature of the research invites the intrusion of illness, death, uncontrolled experiences, or variations in "standard" laboratory conditions, etc., between treatment and test—it is also true that our knowledge of the effects of early experience has suffered from inadequate attention to methodological details which *can* be dealt with. There are six points which are especially crucial.

Meaning or Denotation of Early Experience. As King (1958) and Baron, Brookshire, and Littman (1957) have indicated, the term "early" is vague and has been used to cover a wide age range. If the effects involved are in any way a function of the developmental level of the organisms studied then some more precise way of indicating such levels is necessary. Hence King has suggested, for example, that the rat's life may be broken into three phrases—infantile (0-20 days), adolescent (21-70 days), and adult (71-). By this criterion, for example, the research of Baron, Brookshire, and Littman (1957) which purported to deal with *infantile* experience actually dealt with *adolescent* experience; now the present authors use the term "weanling" to refer to the same early experience phase. It is clear that some unambiguous distinctions are required if the effects of early experience are to be understood.

Previous Experience and Early Experience. Demonstrating that there are differences at maturity between a control and an experimental group exposed to varying early experiences is not a sufficient basis for saying that this is an effect of *early* experience. The theories of Freud and Hebb, for example, assert that experiences early in life have effects which are unique and irreversible. This implies that the same experiences later in life will not have the same consequences. In order to test this, however, it is necessary to include in the design of the experiment an adult-experience control group, a procedure which, as Baron, Brookshire, and Littman (1957) and Ader (1959) point out, has rarely been adopted. As an illustration of the significance of this factor, the study by Baron, Brookshire, and Littman showed that the same traumatic treatment of weanling and adult animals resulted in the same behavior during an adult test, and that the behavior differed from that of untreated control subjects. It is certainly true that such "negative" findings for theories proposing unique effects early in life must be viewed in terms of the particular experimental conditions. Nevertheless, it is a clear warning that an adult experimental group paralleling infantile or adolescent groups must be provided when attempting to determine the effects of early experience on organisms; otherwise, these studies will simply reduce to investigations of the effects of *previous* experience. Previous experience is not an age defined characteristic and as a

theoretical term certainly conveys quite a different sense from early experience.

Comparing Different Investigations. According to King (1958) there has been a "lack of attention to variables other than the one being manipulated" in most early experience studies. It is, therefore, difficult, if not impossible, to compare most studies of early experience with one another in an attempt to cross-validate results. For example, most studies differ in one or more of the following ways—age at time of treatment, age at time of test, duration of treatment or test, interval between treatment and test, etc. Such differences raise one of the most crucial problems confronting psychologists in general, viz., the lack of a rational and lawful taxonomy for differences in task and behavior parameters; in other words, where there are differences in tasks and behavior measures, for example, jumping apparatus, linear or open-field mazes, Skinner-type problem boxes, discrimination boxes, obstruction problems, etc., it is impossible to integrate the conflicting findings that presently characterize the area. Except for the dramatic demonstration that there are negative or positive effects of a general nature attributable to massive restriction or expansion of experience possibilities in early life, detailed comparisons are virtually impossible.

One-Experiment Pattern of Research. An especially characteristic feature of research in this area is the large number of investigators who conduct only one study; further, one rarely finds replications either of one's own work or that of others. This, coupled with small effects and usually marginal probabilities wherever differences are assessed, makes it difficult to know what "weights" to assign to different investigations. As a model contrast, we know a good deal about the relation between early experiences and subsequent hoarding behavior because of the careful design and replications conducted by Hunt and his colleagues (Hunt, 1941; Hunt, Schlosberg, Solomon, & Stellar, 1947; Hunt & Willoughby, 1939) on the one hand and Marx and his co-workers on the other (Marx, 1950a, 1950b, 1952).

Confounding of Test Results. Stemming from the "one-experiment" pattern and the tendency to confuse early with prior experience there is a tendency to use a variety of adult tests on the same animals (Ader, 1959; Griffiths & Stringer, 1952). Under such circumstances, unless there already exists a considerable body of control information, one cannot separate the roles of early and immediately prior experience. It is essential, at the very least, to counterbalance conditions for groups; for example, if one wants to know the effect of, say, Treatment X on maze learning and avoidance learning, all subjects should not be studied first on the maze and then on an avoidance test apparatus.

Limited Analysis. While it is indeed important to demonstrate that there is something about a previous treatment that influences the test behavior of subjects, there is much more that requires doing. If the demonstrated relationships are to be more than just dramatic but isolated findings, it is necessary to tease out the mechanisms involved; that is, what are the independent variables, to what dependent variables are they related, and in what manner? When the answers to such questions emerge we can then say we know which concepts *must* be used, rather than which ones *may* be used.

Overview of Studies

Prompted by the positive findings of an earlier study (Baron et al., 1957), a connected series of six experiments dealing with the consequences of severe electric shock was undertaken. The aims of this series were: (*a*) replication of the previously obtained long-term effects of intense stimulation; (*b*) discovery of the conditions for *introducing* shock residua into the repertoire of organisms; (*c*) specification of some of the *properties* of such shock-induced residua, for example, how long do they last, how may they be altered, are there any critical periods, etc.; (*d*) description of the relationship of these residua to adult *learning* and *emotional* phenomena.

Throughout the report, the terms "residual," "shock residua," and other grammatical variants will be used. This neutral usage has been adopted because there are a number of different ways of conceiving of the phenomenon under investigation. It might, for example, be called "fear," "pain," "anxiety," "emotional habit," etc., but since these and similar terms carry special meanings for different investigators we have elected to keep the report of the experimental findings clear from contaminating allusions. In the discussion section, an attempt will be made to relate this empirical "residual" to some of its putative relatives and their characteristics.

One other introductory comment about the studies should be made. In some respects this research program is isolated from that of other workers in the field. In spite of the obvious relations "by problem" to other work, the studies have been cast within a fixed design which does not match in any substantial detail the designs or procedures of other investigators; this was done to facilitate parametric analysis. Nevertheless, it is possible to draw out some broad relationships between the results of our work and that of other investigators and this will be attempted in the discussion section.

Experiment I. Age and Retention Interval as Factors Influencing the Effect of Trauma. The first study (Baron et al., 1957) had used an interval of 100 days between weanling trauma and adult test for the residual but had only a 1-day interval between adult trauma and test. Since weanling traumatized subjects performed at the same level as adult traumatized subjects relative to controls, it was possible that the initial impact of trauma upon pups is greater than upon adults but dissipates with age, hence resulting in an attenuated residual. Therefore, a 100-day interval between trauma and testing was studied for both weanlings and adults.

Experiment II. Role of Duration and Frequency of Trauma in Establishing Residual. In the initial phases of the research we had arbitrarily selected certain fixed durations and intervals for traumatization. But there is no reason to expect the effects of trauma to be uniform and independent of its duration or frequency.[2] Therefore the consequences of different frequencies and durations of trauma were studied.

Experiment III. Behavior Possibilities for the Animal at the Time of Trauma. It is possible that the effect of intense shock is so massive that almost none of the circumstances surrounding its administration can alter the effect of the residual upon subsequent behavior—the effect is always and everywhere the same for given intensities. To psychologists interested in learning this is an unpalatable possibility; it seems more reasonable to assume that there is some kind of learning at the time the shock is administered and that the effects of this experience may be adaptive, maladaptive, or irrelevant for future behavior as a function of relations between trauma and testing circumstances. Consequently, eight groups were studied where the conditions of shock allowed subjects to behave in different ways.

Experiment IV. Residua and Adult Behavior under Dissimilar and Unstressful Test Conditions. In the event the general hypothesis that trauma leaves residua is true, it is important to know whether the residua require stress to manifest themselves or whether they produce a general "characterological" alteration of large, if not all, portions of the organism's repertoire. This was tested by running subjects in a

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[2] Nor, for that matter of its intensity. However, we leave for the future a report on the effects of shock level for the same trauma and test parameters. Even so, the previous study suggests that a moderate and severe shock at the time of testing do not differ appreciably in their ability to reveal trauma residua. Whether moderate and weak shock are equivalent in establishing residua is another matter entirely, and on this we have no information from our procedures. Nevertheless, the work of Denenberg and his colleagues suggests that the strength of residua varies with that of trauma. Since he studied mice it is possible that such results would not hold for rats; this possibility is enhanced by the fact that he did not find an interaction in rats between infantile handling and avoidance learning to different levels of shock (Denenberg & Karas, 1960) as he did in the mice (Denenberg & Bell, 1960).

closed-field test and comparing activity and exploration for control and traumatized groups.

Experiment V. Residua and Adult Behavior under Similar and Stressful or Unstressful Test Conditions. Part of the answer to how adult behavior under stress and nonstress reflects early experience depends on knowing if the adult test situation must resemble, that is, have cue properties similar to, the trauma situation. Consequently, several different trauma conditions were used and animals subjected to them as weanlings were then tested for the role of environmental cue factors versus more general responsivity changes.

Experiment VI. Residua and Adult Behavior under Nonshock Stress—Mild and Severe Hunger. Also closely related to Experiment IV is the question of whether stress conditions different from those imposed during the weanling period will differentiate between treated and control subjects. Even if shock residua appeared during unstressful test circumstances, it is possible that its effect might be enhanced by other forms of stress. Therefore, animals who had been traumatized with electric shock as weanlings were tested for running speed to a goal box containing food when they were hungry.[3]

General Design

The detailed designs, procedures, and results will be given separately for each experiment. However, there are a number of features common to all the experiments and these will be presented together to aid the reader in grasping the over-all framework of the studies.

Subjects

The total number of *S*s was 330, of which 177 were males and 153 females. *S*s were bred and reared in the University of Oregon laboratories, except those used in Experiment V; all were from the Sprague-Dawley strain.

In each experiment subgroups were formed and balanced for weight, sex, and litter at the time of weaning. With the exception of Experiment IV, which used only male *S*s, all the experiments had an equal number of males and females per subgroup.

The *N* for subgroups was equal in each experiment, though different experiments had subgroups of different sizes. Table 1 shows the *N* for each experiment.

Apparatus

This section will describe all the items of equipment used under two headings, trauma and test. The experiment in which each appeared will be indicated.

Trauma

Shock Source. A neon sign transformer was used to step up the normal line voltage to 7,500 volts. The circuit into which the transformer was wired contained 6-megohms resistance so that the current output was approximately 1.25 milliamperes. While this output was not continuously monitored it was periodically checked and in no case varied more than 3%. The voltage and current levels used were, therefore, very high and, coupled with the relatively low resistance variations attributable to individual differences in *S*s, provided us with a very stable traumatizing agent. There was no commutator in the circuit.

In general, the voltage and current levels used in these studies were considerably higher than those in most other investigations in this area. This provides the occasion for a methodological note, harkening back to some points raised earlier. While there has been a great deal of research upon the properties of various shock circuits in

TABLE 1

Allocations of Subjects per Experiment

Experiment	Total N	Number of subgroups	Subgroup N ($\frac{1}{2}$ male, $\frac{1}{2}$ female)
I	96	12	8
II	40	5	8
III	80	8	10
IV	24	3	8[a]
V	60	6	10
VI	30	3	10
Total	330		

[a] Only male subjects.

[3] In addition to the experiments reported here, there have been several studies dealing with the role of the adrenal glands and the interaction of a tranquilizing agent with trauma and test variables. These investigations are being readied for publication, but the authors will be glad to furnish advance summaries to readers who might be interested in the results.

relation to behavior, there are only the beginnings of a systematic behavioral picture of what happens at given shock levels for naive animals, let alone sophisticated ones. Consequently, the voltage and current levels are the only dependable index we have of the severity of trauma. This makes it essential to consider carefully the properties of different circuits when comparing investigations. What may be described as a "high" or "strong" shock in one study may be "weak" or "low" relative to another study. And, of course, until a systematic body of data exists for the effects of a graded series of shock strengths, how can one compare or integrate the findings of different studies?

Shock Box. This, the main traumatization device, was a windowless black box, 9" wide, 18" long, and 11" high with a grid floor having ⅛" bars, ⅜" apart. It had a hinged top by means of which *Ss* were introduced and removed; there was no light inside the box. It was used in all experiments, I-VI.

Harness. This was a nonconducting masonite board upon which animals were spreadeagled and strapped, ventral surface down, with electrodes attached to the front paws so that current necessarily passed through the body. The device permitted no up-and-down movement and only a minimal kind of motion along the body and limb axes, a "hunching-up," so to speak. The device, with the animal attached, was placed on the top of a laboratory table and then wired into the same current source as the shock box. This was used in Experiments III and V.

Grid Runway. Constructed of masonite with grid floors, the device was 5′ long, 6" wide, and 8" high throughout. It was divided into three portions: a dark brown start box (12" × 6"), a runway (36" × 6"), and a white goal box (12" × 6"). The start box had a grid floor that was continuous with that of the runway while the floor of the goal box was masonite. Between each section was a guillotine-type door controlled by overhead cords. The entire apparatus was housed in a plain room with a single fluorescent light fixture high overhead oriented to the long axis of the device. This was used in Experiments I, II, and III.

Closed Field with Grid. Constructed of masonite, the device was 30" × 30" × 6". The walls were painted black and above the floor, which was white and divided by black lines into 36 5" squares, was a grid for administering shock. The apparatus was raised 3′ from the ground and placed in a room with permanently installed blackout curtains on the windows. A single fluorescent ceiling fixture provided a constant source of illumination. This was used in Experiment V.

Cold Water Tank. A small circular tank, 12" in diameter and 24" deep, with water maintained at 37°F. was used in Experiment V.

Test

Four devices were used for testing: grid runway—described in preceding section; closed field with grid—same as in preceding section; closed field without grid—same as shock box but without grid; and elevated runway—a straight runway, 9′ long, 2" wide, painted black and raised 3′ above the floor.

Scores

There were several sorts of measures used in the various studies. We shall describe here only those for the grid runway test which was used in the first three studies. The other measures will be described in the experiments in which they were used.

Running Time

Five seconds after *Ss* were placed in the start box, the guillotine door separating the start box from the runway was raised, providing a combined visual and auditory CS for the rat. Raising the door initiated a 2-second "delay" period after which the UCS (shock) was switched on by means of an automatic interval timer. Running time was the interval between raising the door and entrance into the goal box; the goal box floor had a pressure-plate switch which stopped the timing circuit started by raising the guillotine door in the start box.

Nontime Measures (Achievement)

Escape Responses. Those responses which occurred after shock onset.

Incomplete Avoidance Responses. Those responses where movement into the runway out of the start box occurred before shock onset but which did not bring the *S* to the goal box before the grid was charged. In this instance, anticipatory behavior occurred but *S* failed to act quickly enough and therefore received primary (shock) reinforcement.

Avoidance Responses. Those responses which permitted *S* to reach the goal box before shock onset.

It should be noticed that *membership in one achievement category excludes membership in either of the other two.*

Procedures

Only those procedures which were followed in all of the studies will be described.

Weaning. All *Ss* were weaned at 20 days of age. From that date on, they were maintained in

front-opening individual cages, mounted in battery racks.[4] Water bottles and bulk feeders were used.

Handling. With the exception of specific experimental procedures requiring contact by manipulation, *S*s remained in their individual cages throughout the experiment.

Weanling Trauma. *S*s were shocked from 21-25 days of age for 2 continuous minutes per day except where otherwise indicated. Experiment II should be consulted for leads as to the parametric implications of this schedule of traumatization.

Escape Learning. Escape learning consisted of those trials early in the learning series before *S*s anticipated shock onset. Five seconds after animals were placed in the start box, the guillotine door leading to the runway was raised and after a 2-second delay the grid was charged. This procedure was not followed in Experiment I; instead of a 2-second delay between raising the door and shock onset, the two occurred simultaneously. There were five learning trials per day for 10 successive days; trials were 3 minutes apart and *S*s remained in the goal box for 5 seconds before being removed.

Avoidance Learning. Avoidance learning consisted of the same procedure described above for escape. A running score of less than 2 seconds meant complete avoidance while a score of more than 2 but less than 4 seconds meant incomplete avoidance. In Experiment I, where the escape procedure was different from all other studies, the avoidance procedure was the same, with the usual 2-second interval. There were five learning trials per day for 10 successive days; trials were 3 minutes apart. *S*s remained in the goal box for 5 seconds before being removed.

The Experiments

This section presents specialized procedures, results, and a brief discussion for each of the experiments. The concluding discussion takes up the implications of the experiments as a group.

Experiment I: Age and Retention Intervals as Factors Influencing the Effect of Trauma

Probably the most important developmental hypothesis of recent years proposes that there are critical periods in the life of the organism; it is only at such times that certain experiences may have any effect at all or may have a maximal effect. Work on imprinting (Thorpe, 1956) and the investigations of Harlow (1958) are very suggestive here. Is this also the case for traumatic experiences? An earlier attempt (Baron et al., 1957) to answer this question yielded negative results, viz., weanling and adult traumatized *S*s did not differ from one another on adult tests though both differed from controls. In that study, age at trauma and interval between trauma and test were confounded, leaving open the possibility that weanling effects are greater than adult effects but dissipate with age if not sustained by other experiences. In this experiment, therefore, the effects of age at trauma and interval between trauma and test are separated out.

Procedure

There were 96 *S*s (48 males, 48 females), all of whom were weaned at 20 days and placed on an ad lib. feeding schedule in individual cages. The shock box and grid runway were used. Table 2 outlines the experimental schedule.

Treatment. Each of the 12 groups was matched with respect to weight, sex, and litter and was formed at the time of weaning. Experimental *S*s received 2 minutes of continuous shock on each of 5 successive days, while control *S*s were neither shocked nor handled. As may be seen from Table 2 each experimental trauma group had a separate control.

Since the events under trauma are so uniform—except in some of the conditions in Experiment III—we present here a general account of what happens when shock is applied.

A 1.25-milliampere current induces frantic running accompanied by urination and defecation. If shock duration is long then running yields to tonic immobility; this immobility is not the result of fatigue, apparently, since upon shock termination *S*s immediately revert to active state.

There is a great deal of leaping, as well as running, under shock; usually this concentrates upon the walls of the box rather than being undifferentiated leaping. By the third or fourth day, *S*s begin to jump immediately on being placed in the box, that is, before shock is applied.

The paws of some *S*s occasionally bleed during trauma; either the frantic running itself or the electrical arcing from the grid could produce this. Since adult animals do not show these injuries it is clearly the more delicate tissue of the pups which makes some of them susceptive to damage.

No permanent defects or deaths which could be attributed to the shock were observed. The hypothesis that a selection process is in operation (whereby only the hardier animals survived the

[4] Cages were No. 409 and racks were No. 410 in the lines manufactured by Bussey Products Company; Chicago, Illinois.

TABLE 2

OUTLINE OF EXPERIMENTAL SCHEDULE FOR EXPERIMENT I

Group	N (Total = 96)	Age at time of treatment (trauma)	Age at time of testing	Testing situation
E I_a	8	20–24 days	125–132 days	Escape
E I^b	8	20–24 days	125–132 days	Avoidance
C I_a	8	(No shock)	125–132 days	Escape
C I_b	8	(No shock)	125–132 days	Avoidance
E II_a	8	120–124 days	125–132 days	Escape
E II_b	8	120–124 days	125–132 days	Avoidance
C II_a	8	(No shock)	125–132 days	Escape
C II_b	8	(No shock)	125–132 days	Avoidance
E III_a	8	120–124 days	225–232 days	Escape
E III_b	8	120–124 days	225–232 days	Avoidance
C III_a	8	(No shock)	225–232 days	Escape
C III_b	8	(No shock)	225–232 days	Avoidance

trauma to be tested as adults) is certainly not tenable.

Finally, it is apparent that taming occurs during the infantile treatment phase of the experiment. Not only do the handled groups adapt to the experimenter (E) during the course of treatment, but the shock groups also do. It should be emphasized, however, that with regard to the shocked animals this "tameness" is evident only before the daily treatment. Immediately after treatment *S*s are difficult to handle, making explicit escape responses on being picked up by E.

Test. *S*s were given the escape and avoidance training described in the section on General Design. It should be noted that this experiment is the one in which escape training involved simultaneous shock and door raising, in contrast to the remaining experiments. There were five training trials per day for 8 successive days; trials were spaced 3 minutes apart.

Results

The results for escape and avoidance learning are presented in Table 3.

Escape Learning in "Escape Situation." Escape behavior was analyzed for the early trials, 1-5, and later trials, 6-40. An analysis of variance for Trials 1-5 with experience (shock or no-shock), trauma-test interval, and sex as primary sources reveals *trauma-test interval* and the *trauma-test interval* × *experience interaction* to be significant at the 5% level. By Tukey's gap test it would appear that the group contributing most to the interaction variance is Group E II_a (see Tables 2 and 3), that is traumatized on Days 120-124 and tested immediately afterward beginning Day 125 until Day 134. This group acquires the escape running habit under shock more quickly than either the control groups or *S*s exposed to shock 100 days prior to escape training (regardless of age at time of trauma).

The analysis of variance for Trials 6-40 reveals only one source significant at the 5% level, *experience*. Inspection of Table 3 shows that traumatized *S*s in all three groups have longer running times than their nonshocked controls.

Escape Reactions in the "Avoidance Situation." The early trials of avoidance learning are very much like escape learning conditions since anticipatory responses have to be developed. This may be seen from Figure 1 which is a plot of the three types of responses in the avoidance situation. It is not until the fifth day of testing that avoidance frequencies approach escape-type behavior. For the first day—Trials 1-5—there are never any complete avoidances though occasional incomplete avoidances appear; these data are based on the *S*s in Experiment III. Hence, we have analyzed the avoidance trials

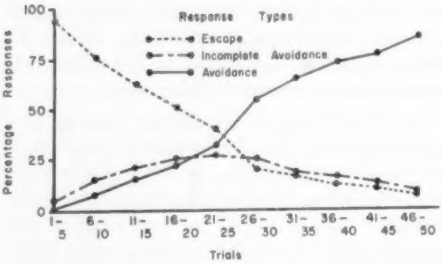

FIG. 1. Frequency of response types as a function of testing.

in which the response did not occur until the grid was charged, so that running was to escape shock. It can be seen from Table 3 that the escape behaviors under avoidance operations have the same pattern that they do under escape operations, though Trauma Groups I and III do somewhat better (probably revealing some influence from the growth of anticipatory behavior). In any event, an analysis of variance reveals *sex* and the *trauma-test interval interaction* to be significant sources of variance. When the Tukey gap test is applied, it is seen that the E II_a (immediate test) group escapes more rapidly than any other experimental or control group, replicating the findings for the "pure" escape situation.

Number of Incomplete Avoidances. In Table 3 it may been seen that the experimental *S*s consistently have more incomplete avoidances than the controls, that is, running before shock begins but failing to reach goal box before shock onset. However, an analysis of variance did not yield any significant sources of variance, intragroup variability being very great.

Number of Complete Avoidances. Table 3 shows how the groups compared on complete avoidances. The superiority of experimental animals over controls (paralleling those in the incomplete avoidance measure) stands up here; an analysis of variance shows both experience (traumatized or nontraumatized) and *sex* to be significant sources. Note that here there is no interaction, such as found for escape responses, between trauma and test interval.

Age at Traumatization. Whether or not the age at which traumatization occurred is related to residual effects is one of the major objectives of this experiment. The

TABLE 3

RESULTS FOR ESCAPE AND AVOIDANCE IN EXPERIMENT I

Situation	Subjects E(xperimental) C(ontrol)	Trials	Trauma-test interval group		
			I (20–125)	II (120–125)	III (120–225)
Escape					
Mean running time (seconds)	E	1–5	5.47	2.27*	4.85
	C	1–5	3.71	3.85	4.19
	E	6–40	1.75	1.72	1.95
	C	6–40	1.49	1.50	1.55
Avoidance					
Mean running time in seconds for escape-type responses (includes 2-second delay)	E	1–5	5.74	4.37*	5.60
	C	1–5	5.64	5.82	5.55
Mean number of incomplete avoidances	E	1–40	7.62	7.78	8.50
	C	1–40	5.00	6.62	6.00
Mean number of avoidances	E	1–40	8.88	10.62	10.25
	C	1–40	6.75	4.62	7.00

* Differs at 1% level by Tukey gap test from other E and C groups.

answer is clear-cut: *there is no effect attributable to age at traumatization.* This may be seen by comparing the behavior of animals traumatized at 20-24 days (Group I) and 120-124 days (Group III), for whom the interval between trauma and test is equal though the age at time of test is not. As will be seen from Table 3, the running times and relative frequency of avoidance behaviors are of the same order of magnitude for both groups. It should be noted that the experimental *S*s do, of course, differ from the controls.

Age at Time of Testing. If we inquire whether the age at test is related to traumatization experiences, the answer is also negative. It will be seen from Table 3 that whether animals are tested at 125 days of age or 225 days they behave substantially the same. This particular generalization is clouded by the superior escape behavior of Group II (the "immediate" group) in the first five trials; the difference seems to depend upon the interaction of test age × trauma-test interval according to the analysis of variance referred to above. Since the interaction dissipates when the full range of 40 trials is considered it seems safe to conclude that the test age is an irrelevant variable.

Interval between Trauma and Test. Here we find the only instance in which the experimental groups are split off from one another relative to the control groups. Group II, the immediate group, shows better escape behavior than the other experimental animals but also is superior to the control *S*s. *This point should be kept in mind because it is the only instance in this entire series that traumatized animals show superior escape behavior.* The effect of a brief trauma-test interval does not appear in connection with avoidance behaviors where, as will be seen in all subsequent experiments, experimentals do better than controls. Therefore, our data suggest that long intervals between trauma and test do not alter the influence of trauma residuals, regardless of the age at which traumatization or testing occurs. Short intervals, on the other hand, seem to make the residual advantageous for escape behavior, though only for a brief period, since the mean escape behavior for Trials 6-40 shows Group II behaving at the same level as the other two experimental groups (Table 3).

Sex Differences. We do not present any tables in which sex differences are analyzed. However, females behaved more poorly than males under both escape and avoidance conditions. This was true for both control and experimental *S*s and since there is no interaction between sex and any of the other variables it probably reflects nothing more than the general slowness of running in female rats relative to males.

Conclusions

1. The effects of trauma upon weanlings are the same as its effect upon adult animals; escape learning is hampered and avoidance learning is aided. If there is a "critical" period, it is somewhere before Day 20 or between Days 35-120.

2. Trauma residuals operated upon 125- and 225-day-old animals to the same degree.

3. An interval of 100 days between trauma and test has the same influence whether trauma occurs in weanlings or adults.

4. There is a transitory benefit for escape if testing occurs a short time after trauma.

5. The effect of traumatization, that is, the influence of residuals, may be advantageous or disadvantageous depending upon the task confronting the *S*. There is an interaction between residuals and behavior setting so that there can be no maxims to the effect that "Trauma is harmful (independent of test circumstances)" or that "Trauma is beneficial (independent of test circumstances)."

Experiment II: Role of Frequency and Duration of Shock in Establishing Residua

Once it was clearly established that a residual of shock-trauma did indeed exist, the next logical step was to explore the conditions under which the trauma was administered. We first turned our attention to the frequency and duration of the shock, leaving variations in shock intensity for a

future series of replications. The need for this sort of parametric investigation is obvious, for it is perfectly possible that we had just happened to hit upon a lucky combination of duration and frequency of trauma; if that were so, then it would be necessary to alter the strategy of our thinking considerably, shifting to a study of just why only a narrow range of values was successful. To anticipate the results, it turned out that there is a wide range within which residua may be established.

The hypothesis underlying Experiment II was that the strength of the residual is linearly related to the frequency and duration of trauma. As will be seen, however, the hypothesis was only imperfectly tested by the design used; for duration there were three points but for frequency only two points.

Procedure

There were 40 *S*s (20 males, 20 females) bred in the University of Oregon laboratories. The shock box and grid runway were used. Table 4 outlines the experimental schedule for the five groups of eight *S*s each.

Weanling Treatment. *S*s were kept in individual cages starting with Day 20 (weaning) and thereafter maintained on ad lib. feeding and watering. After the weanling trauma period the animals were not handled until they reached 100 days of age, at which time they entered upon the adult testing phase of the experiment.

Adult Testing. Testing consisted of five training trials per day for 10 consecutive days on the grid runway. Escape testing consisted of the escape-type behaviors which occurred during the first five trials of avoidance training operations. No control groups were used though comparisons with the behavior of control animals in other experiments in the series will be offered.

TABLE 4

OUTLINE OF SCHEDULE FOR EXPERIMENT II

Group	Exposure per day (in minutes)	Number of days	Age of subjects
A	1	5	21–25
B	2	5	21–25
C	4	5	21–25
D	2	10	21–30
E	4	10	21–30

Results

The results for escape and avoidance are given in Table 5.

Escape. An analysis of variance indicates that both duration of exposure per day and number of days are significant sources of variance ($p < .05$). Tukey's gap test suggests that the *S*s with 1 minute of shock are superior in escape behavior to all others, but that whether animals receive 2 or 4 minutes of shock per day does not matter. It is also evident that animals shocked for 10 days do more poorly than those shocked for only 5 days, that is, have longer running times.

Comparison of these results with those for the control animals in Experiment I (see Table 3, Avoidance, escape-type responses, Control group scores) makes it clear that the differences between 5- and 10-day groups occur at a level which is lower than that for control groups. In other words, a residual effect is present so that both 5- and 10-day groups escape more slowly than control *S*s. Comparisons may also be made with control data for the experiments reported below; they, too, will show that a residual effect is present in the behavior of these traumatized *S*s.

Incomplete Avoidance. The analysis of variance does not yield any significant sources of variance for the data in Table 5.

Complete Avoidance. The analysis of variance indicates that there are no significant differences among the three groups whose data are given in Table 5.

Conclusions

Once again, it was demonstrated that there is a residual effect of weanling trauma upon adult escape and avoidance learning. In addition to differing among themselves, the *S*s in this experiment do more poorly on escape than control animals in other studies.

Duration. One minute of shock per day for 5 days does not produce a residual that

TABLE 5

EFFECT UPON ADULT ESCAPE AND AVOIDANCE BEHAVIOR OF DIFFERENT DURATIONS AND FREQUENCIES OF WEANLING TRAUMA
(Experiment II)

Test situation	Days of shock	Daily duration of shock trauma (minutes)			
		1	2	4	Mean
Escape					
Mean running time in seconds (Trials 1–5)	5	5.73*	7.35	7.20	6.76
	10		9.38	7.97	8.68
	M	5.73	8.36	7.59	
Avoidance					
Mean number incomplete (Trials 1–50)	5	9.71	7.50	9.25	8.82
	10		7.78	10.38	9.08
	M	9.71	7.64	9.82	
Mean number complete (Trials 1–50)	5	25.00	22.50	23.50	23.67
	10		22.00	20.00	21.00
	M	25.00	22.25	21.75	

* Differs at 5% level by Tukey's gap test from other four groups.

appears in escape behavior. Whether it would if prolonged for 10 days we cannot now say. It is clear, however, that increasing the exposure does produce a residual, though there is no difference between 2 minutes as compared with 4 minutes. This differential effect occurs only for escape behavior. As far as avoidance behavior is concerned, merely being shocked seems to be the relevant factor; this can be seen by comparing the results for these *S*s with the behavior of controls in Experiment I or any of the experiments below.

Frequency. Ten days of shock produce slower escape times than 5 days. Avoidance behavior is not altered by increasing the number of days on which shock is given, though the absolute scores are in the direction of lowered avoidances with increased number of days.

Linearity Hypothesis. The data do not support such an hypothesis. A more reasonable hypothesis is that there is a quantum or threshold phenomenon: Once a particular duration and frequency of shock is reached, there are no variations in the relation between escape and avoidance behavior as a function of increases in frequency or duration of trauma. It should be kept in mind that these results are for a single, very high, shock level. It will be interesting to see what the results are for a replication of this design using lower shock levels.

Differential Impact on Escape and Avoidance. As in all the experiments in the series, whenever trauma has occurred, escape behavior is less, and avoidance behavior is more, efficient. However, in this investigation, it is also true that escape behavior shows an influence from differing durations and frequencies of shock while avoidance behavior does not. The two patterns of behavior are apparently under different "controls."

Experiment III. Behavior Possibilities at the Time of Trauma

While the effect of shock trauma upon animals might be so massive as to flavor thereafter large areas of the animal's behavior, it need not be so. It is possible that there are fairly uniform patterns of responding to

shock which become involved in some kind of emotional or instrumental learning even at the weanling stage and which are subsequently responsible for the apparent effects of shock. With this possibility in mind—it might be dubbed the *learning* viewpoint—there was an attempt to control directly and indirectly some of the things that weanlings might do when they were shocked; the behavior of the *S*s under the standard adult test conditions was then analyzed in relation to the early behavior possibilities made available to them by our experimental procedures. In all, there were eight different conditions to which *S*s were exposed, only one condition, of course, to an *S*.

Procedure

There were 80 *S*s (40 males, 40 females) from the Oregon colony. All animals were weaned at 20 days of age and placed on an ad lib. feeding schedule in individual cages. The shock box, grid runway, and harness were used. There were 8 groups of animals. As usual there were two phases, a weanling treatment and adult test.

Weanling Treatment. From Day 21 to Day 25 each *S* was subjected to one of the following treatments.

1. Group E_1 (complete escape)—two subgroups.

a. E_{1a}. *S*s were trained to escape shock by running across 3′ of charged grid to the uncharged goal box. The *S* was returned to its living cage after the correct response was made; there was a 3-minute intertrial interval. Total amount of exposure to shock was controlled by subjecting each *S* to that number of daily trials which produced a cumulative total of 2 minutes on the shock grid.

b. E_{1M} (maturational control). Because *S*s were young there was reason to expect a strong maturational influence upon speed of running. Therefore, a separate group of six animals, when they were 25 days old, was given 1 day of escape training exactly as done with the main group. A comparison of the performance for the first day of learning of the 25-day-old animals with that for the first day of the 21-day-old animals would provide an index of the advantage attributable to maturational influences.

2. Group E_2 (start box—no escape). *S*s were exposed to electric shock only in the start box of the grid runway. No escape was possible. Each animal was matched with a littermate in Group E_1, whose length of exposure to shock per trial was duplicated. As in Group E_1, at the termination of each trial *S*s were returned to their living cages, and trials were spaced 3 minutes apart.

3. Group E_3 (start box—intermittent shock). *S*s in this group were also exposed to electric shock in the start box of the grid runway. Once again, each animal was matched with a littermate in Group E_1, whose length of exposure to shock per trial was duplicated. However, instead of being returned to its living cage following each trial, the E_3 *S* was required to remain in the start box for 20 seconds, following which the next trial began.

4. Group E_4 (start box—continuous shock). *S*s received 2 minutes of continuous shock per day in the start box of the grid runway. Each *S*, therefore, received only one "trial" per day but a total duration of shock equal to that of the preceding groups.

5. Group C_1 (handled). Each *S* was removed daily from its home cage and placed for 2 minutes in the start box of the runway but was not exposed to shock.

6. Group E_5 (shock box—continuous shock). *S*s were exposed to 2 minutes of continuous shock per day in a black box which contained no windows or any other light source.

7. Group E_6 (harness—continuous shock). *S*s received 2 minutes of continuous shock per day in a harness arrangement permitting little, if any, movement of the skeletal musculature. All four legs, as well as the abdomen, were secured to a nonconducting masonite board. Electrodes were attached to the forepaws of the *S*, so that the current necessarily traveled through the abdomen.

8. Group C_2 (ignored). *S*s were not removed from their cages at any time after weaning on Day 20 until testing was initiated 80 days later.

Adult Testing in Grid Runway. Avoidance training for *S*s in each of the eight groups commenced at 100 days of age. *S*s were given five training trials per day for 10 successive days. Trials were spaced 3 minutes apart.

Results

There are two sets of results, those for the learning of the E_1 group weanlings and for all animals during the adult tests.

E_1 Weanling Learning. The main purpose of this group was to see whether early direct training on the task used for adult testing would have positive transfer value. Consequently, it is essential to know whether any learning occurs during the weanling period because, after all, the *S*s are very young.

The results are quite definite; weanling animals learn to run faster under escape

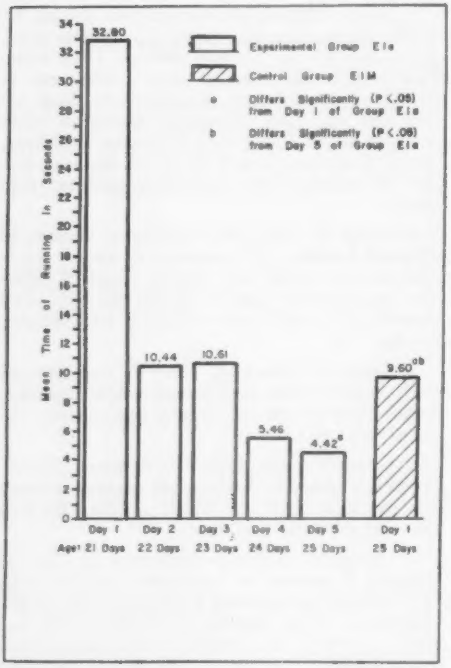

FIG. 2. Mean running times of escape group (E_{1a}) and its control (E_{1M}). (Experiment III)

conditions. Figure 2 shows the changes in running time over the 5 days of testing for the *S*s of E_{1a} and E_{1M}. The performance of E_{1a} for Day 25 is significantly superior to its performance on Day 21 by the Mann-Whitney *U* test. However, the performance of the maturation control group, E_{1M}, on its first trial which took place on Day 25, is also superior to the first day performance of the E_{1a} group, indicating that there is a clear-cut maturational effect at work; in this case, undoubtedly, a simple increase in ability to run swiftly. At the same time, the E_{1a} group is superior on its last day of training to the first day of the E_{1M} group, so it is safe to conclude that while maturational factors account for a tremendous portion of the increase in speed for Day 25 over Day 21, it does not account for all of it; percentagewise, the drop from 33 seconds on Day 1 to about 5 seconds on Day 5 is about 79% maturational and about 21% the effects of the learning trials.

We can be quite sure, therefore, that *the weanling animals of E_{1a} learned an escape running habit.*

Adult Testing. The results are presented separately for escape and avoidance.

1. Escape: Table 6 gives the group mean running times for escape responses on Trials 1-5 and 6-50; the data are ordered by running time and divided in accordance with the results of the Tukey "layering" test for Trial 1-5 (Ryan, 1959). It should be noted that the escape responses for Trials 6-50 are based on markedly different numbers of trials since such responses are interspersed among increasingly frequent avoidance-type responses (cf. Figure 1). However, a previous study has shown that no relationship exists between escape times and the number of trials on which they are based after asymptotic levels are reached.

The analysis of variance for Trials 1-5 reveals *infantile experience* is a significant source of variance. The analysis of variance for Trials 6-50 does not show experience to be a significant source. In both analyses of variance sex was a significant source, with

TABLE 6

ESCAPE RUNNING TIME DURING ADULT TEST DIVIDED INTO TWO CATEGORIES, FOR TRIALS 1-5 ONLY, BASED ON TUKEY'S "LAYERING" TEST (Experiment III)

Category	Groups (weanling experience)	Trials 1–5	Trials 6–50
High running speed	E_1 (escape training)	4.05	3.28
	C_1 (handled)	4.81	3.37
	C_2 (ignored)	4.83	3.27
Low running speed	E_5 (shock box—continuous shock)	6.40	3.77
	E_2 (start box—no escape)	6.42	3.72
	E_6 (harness—continuous shock)	6.62	3.73
	E_3 (start box—intermittent shock)	6.63	4.09
	E_4 (start box—continuous shock)	7.18	4.09

TABLE 7

Complete Avoidance Behavior during Adult Test, Divided into Two Categories (Mean Frequency of Avoidance Only) According to Tukey's "Layering" Test (Experiment III)

Category	Groups (weanling experience)	Mean avoidance frequency	Mean trial until first avoidance
Most avoidances	E_1 (escape)	27.6	11.2
	E_3 (start box—intermittent shock)	24.2	16.3
	E_5 (shock box—continuous shock)	23.0	17.3
	E_2 (start box—no escape)	22.7	16.5
	E_4 (start box—continuous shock)	22.5	20.2
Least avoidances	C_2 (ignored)	18.6	19.6
	C_1 (handled)	18.5	19.6
	E_6 (harness—continuous shock)	18.3	19.8

males being faster than females; such a difference does not exist in the one weanling group for which we have behavioral data, E_1.

As the configuration resulting from the Tukey test indicates, animals who received direct escape training in infancy were, unlike other traumatized animals, able to escape as efficiently as control animals. The other traumatized animals had slow escape times. It is interesting to note that the results for Trials 6-50 follow the same ordering, though, because the analysis of variance did not show a significant *F*, the Tukey test has not been applied.

2. Avoidance: Since Incomplete Avoidance (IA) scores showed no differences among groups for two measures—"mean IAs per group" and "mean trials until IA"—we do not present the IA data.

These same measures were applied to Complete (or successful) Avoidances and the results are shown in Table 7, which has been organized in the same manner as Table 6 for escape responses. Significant differences for the early experience and sex sources are found for mean number of avoidances only; while the mean trials until first avoidance are in the same direction, they do not yield a significant *F*. The configuration resulting from Tukey's layering test is different from that for escape behavior and we draw special attention to these differences because they are the basis for some of our conclusions concerning the nature of shock trauma. All traumatized *S*s do better than controls except for the harness group (E_6); the control groups are joined by the animals restrained when traumatized in giving poor avoidance scores.

Conclusions

1. As in the previous two studies, trauma generally hinders adult escape learning and facilitates avoidance learning.

2. However, direct training in the ultimate test device, if coupled with trauma, confers an advantage in both escape and avoidance learning.

3. While it is true that trauma generally impairs escape and benefits avoidance behavior, there are at least two conditions where it is not the case. Both are related to the behavior possibilities available to the *S* at the time of trauma. Point 2 above indicates than when positive transfer training is involved, trauma is advantageous. At the other end of the scale, when no substantial instrumental behavior is permitted the *S*s as in the case of the harness group, E_6—then escape behavior is disadvantaged but, also, avoidance learning is impaired; restrained animals who are shocked learn to avoid at the same slow rate as control animals who have never been shocked.

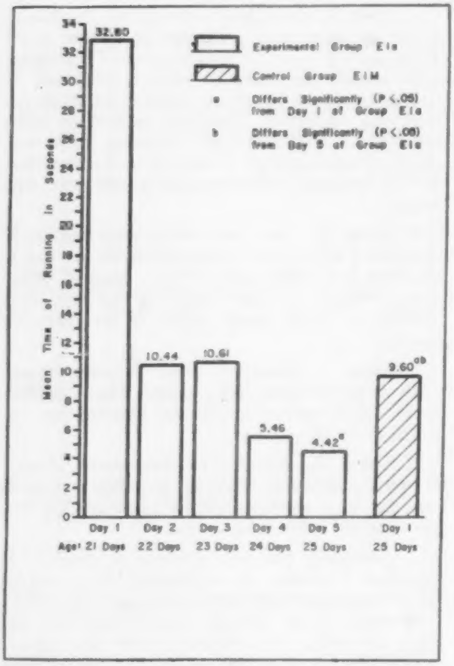

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TABLE 7

COMPLETE AVOIDANCE BEHAVIOR DURING ADULT TEST, DIVIDED INTO TWO CATEGORIES (MEAN FREQUENCY OF AVOIDANCE ONLY) ACCORDING TO TUKEY'S "LAYERING" TEST (Experiment III)

Category	Groups (weanling experience)	Mean avoidance frequency	Mean trial until first avoidance
Most avoidances	E_1 (escape)	27.6	11.2
	E_3 (start box—intermittent shock)	24.2	16.3
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3. While it is true that trauma generally impairs escape and benefits avoidance behavior, there are at least two conditions where it is not the case. Both are related to the behavior possibilities available to the *S* at the time of trauma. Point 2 above indicates than when positive transfer training is involved, trauma is advantageous. At the other end of the scale, when no substantial instrumental behavior is permitted the *S*s as in the case of the harness group, E_6—then escape behavior is disadvantaged but, also, avoidance learning is impaired; restrained animals who are shocked learn to avoid at the same slow rate as control animals who have never been shocked.

Experiment IV: Residua and Adult Behavior under Dissimilar and Unstressful Test Conditions

The most general finding of Experiment III was that traumatic shock experience produces a nonspecific residue although instrumental conditioned responses may also be acquired in certain specific instances. If this nonspecific residue is thought of as an emotional change, a sensitization to external stimuli, it should influence a very wide range of behavior. In order to test this possibility, a closed-field test was chosen as the test situation in Experiment IV.

Notice that in this, as well as subsequent experiments, shock treatment was carried out with the windowless black box, although from Experiment III it is evident that the details of the situation are not important, so long as *S* is not given an opportunity to escape the shock.

Procedure

*S*s were 24 albino rats from the University of Oregon colony. All *S*s were males. At 20 days of age each animal was weaned and placed in a separate cage, and assigned to one of three groups of eight animals, matched with respect to litter and weight. For the infantile treatment, the apparatus used was the shock box. For the adult test, the apparatus used was the closed field.

Infantile Treatment. The experimental procedures used for infantile treatment were replications of the procedures used for Groups E_4, C_1, and C_2 of Experiment III. Thus, *S*s were either shocked, handled, or ignored in infancy.

Adult Test. From 100 through 109 days of age the three groups of rats used in this experiment were exposed to the closed-field test situation. Each *S* was given a single daily test trial of 10 minutes. The number of spaces traversed was recorded separately for each minute of the trial. At the conclusion of the trial, after *S* had been returned to its living cage, the number of fecal boluses and the number of spots of urination were recorded, after which the apparatus was cleaned with a sponge. To minimize handling, each *S* was transported to and from the test room in its living cage. The ad lib. feeding and watering conditions under which *S*s had been maintained since weaning were not changed during the course of adult testing.

Results

Activity. In Figure 3a intertrial activity of each group is presented. Here each point represents the mean number of spaces traversed in a single, daily 10-minute test session.

Figure 3b shows group intratrial activity. Each point in this case represents the mean number of spaces traversed during Minutes 1, 2, 3, etc., when scores are summed over trials.

By inspection it is evident that in both graphs the individual curves have an overall negative slope. When dichotomized scores for each *S* were used, 18 out of 24 *S*s were more active during Trials 1-5 ($p < .05$ by means of the Sign test—Siegel, 1956). Likewise, the total number of squares traversed by each animal on Minutes 1-5 was greater than during Minutes 6-10 for 20 out of the 24 *S*s ($p < .01$ by means of the Sign test).

It appears, then, that activity decreases as a function of exposure to the closed-field situation; this reduction can be observed within a given trial, but also occurs between trials spaced 24 hours apart. These data closely parallel those collected by other experimenters (Berlyne, 1955; Welker, 1956) on exploration.

Analyses of variance performed on the activity data indicate that no *group* differences exist with regard to activity levels early or late in the test trial or early or late in the series of trials.

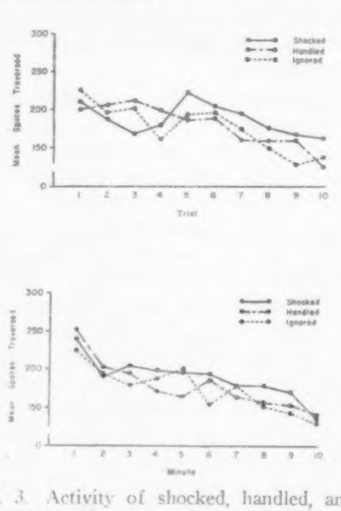

FIG. 3. Activity of shocked, handled, and ignored groups. (3a—upper—Intertrial; 3b—lower—Intratrial. Experiment IV)

Urination. The total number of urine spots over 10 days observed for the shocked group was 25, for the handled group 24, and for the ignored group 32. The Kruskal-Wallis H test for nonparametric data (Siegel, 1956) indicated no differences between conditions.

Defecation. Sixteen out of the total 24 rats did not defecate at all during testing in the open field. Of the eight *S*s who did defecate, three had previously received shock, three had previously received handling, and two had been ignored. The Kruskal-Wallis H test indicated no significant differences between conditions.

Conclusions

1. The results of Experiment IV were conclusively negative: neither intense electric shock nor handling in infancy affected subsequent behavior in the closed field. Furthermore, the data presented on urination and defecation discourage further use of these measures as indicators of moderate degrees of emotionality. Response frequency was simply too low to provide for adequate determination of individual differences. As has been previously suggested (Hunt & Otis, 1953), the Hall tests may be reliable only when anxiety level is relatively high.

2. These negative findings reduce the scope of the hypothesis concerning nonspecific residua: intense stimulation administered in infancy clearly does not influence the total range of adult behavior in rats.

3. Similarly, the consequences of handling may be limited, although once again our handled *S*s received less total early experience than *S*s used in other experiments.

Experiment V: Residua and Adult Behavior under Similar, Stressful, and Unstressful Test Conditions

Experiment III showed that *S*s may display at least some of the effects of traumatic shock even when no instrumental behavior is permitted during treatment (cf. harness group, E_6). However, it is clear from Experiment IV that the residua induced by shock is not a pervasive or highly general effect. There are two possible ways in which the residue might operate in adults: (*a*) it might be closely keyed to the occurrence of shock, in which case shock might serve as a drive or cue factor; (*b*) there might be an environmental feature of the trauma situation which acquires either drive or cue value.

In Experiment V, therefore, both treatment and test conditions were varied in such a way as to (*a*) maximize or minimize transfer from treatment to test, (*b*) compare different varieties of original stress (electric shock vs. cold), (*c*) compare adult performance with and without stress, and (*d*) compare the effects upon activity and response thresholds.

Procedure

*S*s were 60 Sprague-Dawley rats from the University of New Brunswick colony. *S*s were weaned at 20 days of age and placed on an ad lib. feeding schedule in individual cages. The shock box, the "white" closed-field box with a grid, the cold water apparatus, and the harness were used.

Treatment. From Day 31 to Day 40 each *S* was exposed to one of the following treatments.

1. Group B (shock box—black). *S*s were exposed to electric shock 4 minutes per day for 10 consecutive days. The *S*s in this group were treated identically with those of Group E_3, Experiment III.

2. Group W (white box). *S*s were exposed to electric shock in identical fashion to those in Group B, except that treatment occurred in the white closed-field apparatus instead of the black shock box.

3. Group CW (cold water). *S*s were exposed to 37°F. water 10 minutes per day for 10 consecutive days.

4. Group H (harness). *S*s in this group received treatment identical to that of Group E_6, Experiment III.

5. Group C (handled). *S*s were placed in the black shock box each day of the treatment period for 4 minutes, but were not subjected to shock.

6. Group C_2 (ignored). *S*s were neither handled nor shocked at any time after weaning until 100 days of age, when adult testing began.

Adult Testing. Commencing at 100 days of age, *S*s from each of the six groups received 6 days of testing. The tests were of three types: (*a*) observation of activity in the white shock box under no-shock test conditions, (*b*) observation of activity in the white shock box while exposed to 1.25 milliamperes of shock, (*c*) determination of re-

TABLE 8

SCHEDULE OF TESTING FOR SUBJECTS OF EXPERIMENT V

Test day	Apparatus	Presence of shock	Length of session (in minutes)
1	White box	No	5
2	White box	No	5
3	White box	Yes	5
4	White box	No	5
5	Black box	Yes	Indeterminate
6	Black box	Yes	Indeterminate

sponse threshold in the black shock box by the method of limits. Table 8 shows the exact testing schedule.

On Testing Days 1-4, activity was measured, as in Experiment IV, by counting the number of 6-inch squares traversed by *S* during the session.

On Testing Days 5 and 6, response thresholds were measured, using the technique described by Kimble (1955). Two observers judged the responses of each *S* as falling into three categories: "jump," "flinch," and "no response" (see Kimble for a definition of these categories). The method of limits was used. Both the ascending and descending series contained the following values (in milliamperes): .05, .10, .15, .20, .25, .30, .40, .50, .60, .70, .80, .90, 1.00. *S*s received one ascending and one descending series on each of the 2 days of testing. The response threshold for each animal was taken as the mean threshold of the four runs. A "jump" threshold was also determined, using the same statistical procedures.

The thresholds were studied to test two hypotheses. One may be called the hypothesis of instrumental response learning during shock. It is possible that when animals are shocked they acquire responses which mitigate the severity of the shock. In that case, when tested later for escape and avoidance under shock, these mitigating responses should be expected to reappear, and, depending upon the task, either facilitate or hinder escape and avoidance.

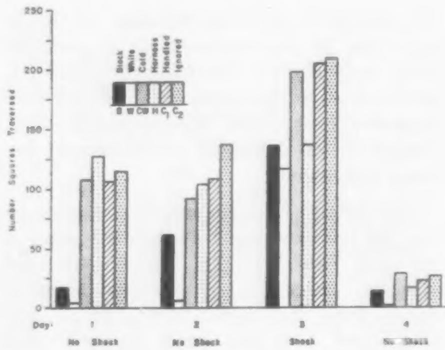

FIG. 4. Activity as a function of test conditions. (Experiment V)

The other hypothesis is that there is a general change in reactivity as a result of shock. In that event, when shocked, the experimental groups which had previously experienced shock should all behave differently from "inexperienced" groups. It should be noted that our design does not permit us to distinguish between these two hypotheses; however, it does permit us to determine if the general outcome they both predict—different behavior under shock for traumatized animals—is the case.

Results

Figure 4 shows the mean activity scores in the white box for *S*s in each group as a function of testing sessions. A separate analysis of variance was performed on the data for each testing session. Table 9 is a summary of these analyses.

TABLE 9

SUMMARY OF ANALYSIS OF VARIANCE OF ACTIVITY IN WHITE SHOCK BOX FOR TEST DAYS 1, 2, 3, AND 4

(Experiment V)

Source	Day 1		Day 2		Day 3	Day 4
	p^a	p^b	p^a	p^b	p^a	p^a
Treatment	$<.001$	$<.05$	$<.001$	$>.05$	$<.001$	$>.05$
Sex	$<.001$	$>.05$	$<.001$	$<.05$	$>.05$	$<.001$
Interaction	$<.01$		$<.001$		$>.05$	$>.05$

[a] Tested against residual.
[b] Tested against interaction.

The Tukey test indicates that Groups B and W displayed significantly less activity on Day 1, and that Groups B, W, and H displayed significantly less activity on Day 3, when performance was measured under shock. The treatment source was not statistically significant on Days 2 and 4.

Table 10 shows the mean response and jump thresholds for the various treatment groups, as determined on Testing Days 5 and 6. Analysis of variance indicated that treatment was not a statistically significant source of variance for either measure.

Conclusions

1. Since *both* Groups B and W show "fear," that is, low activity, on Day 1, the relevant cue must be the *grid bars,* as that is the one cue each had in common.

2. Since Groups B, W, and H all show relative inactivity under shock (Day 3), the important factor for this effect must be the only common element during treatment: *electric shock.*

3. The results for handling and exposure to cold are congruent with the field test results of Experiment IV; there is no indication that either treatment affects exploratory behavior. Hence, the effects appear to be specific to shock.

4. The "fear" effect demonstrated on Day 1 is easily masked. On Day 4, after all *S*s had been exposed to shock the previous day, no significant differences for treatments were found.

TABLE 10

MEAN RESPONSE AND JUMP THRESHOLDS (IN MILLIAMPERES) BY METHOD OF LIMITS ON TESTING DAYS 5 AND 6 IN THE BLACK SHOCK BOX
(Experiment V)

Group	Response threshold	Jump threshold
Cold	.165	.282
White	.142	.285
Black	.155	.274
Harness	.178	.291
Handled	.183	.318
Ignored	.225	.298
	.175	.291

5. The negative response threshold data indicate that is is unlikely that differences in adult behavior may be explained in terms of a responsivity notion. If there are relatively permanent changes of this sort resulting from trauma they are either more subtle or different in kind from those measured by Kimble's procedures.

Experiment VI: Residua and Adult Behavior under Nonshock Stress—Mild and Severe Hunger

In Experiment VI the possibility that intense infantile stimulation might have general effects was studied. The negative results of Experiments IV and V might be attributable to the lack of reliability or validity of the test measures. Further, the closed-field test is typically conducted in the absence of any known motivation (except exploratory drive), and it could be that the effect would exhibit itself only under testing conditions of more substantial motivation. The work of Malmo (1957) with humans suggests this possibility; his studies with psychiatric patients suffering from pathological anxiety indicated that the *resting levels* of physiological function of patients usually could not be distinguished from normal *S*s. Rather, differences existed only when *S*s had been exposed to stressful situations. Thus, in the present experiment, *S*s were required at the time of testing to traverse an elevated runway for food under either a mild or severe hunger drive. It was assumed that hunger was distressing to the *S*s, yet both the "stressor" and the test situation would be totally different from those used in the earlier treatment of the animal.

Incidentally, the weight gains of each group were determined, in order to attempt to replicate the transient weight differential reported by Scott (1955); the present experiment, however, used a shock level four times as intense.

Procedure

Sixty albino rats from the University of Oregon colony were divided into three groups of 20 each. Each group contained an equal number of males and females, matched with respect to litter affiliation and weight. *S*s were weaned at 20 days of age and placed in separate cages. For the infantile treatment, the windowless black shock box was again used to shock infant *S*s. For the adult test, the runway apparatus was used.

Infantile Treatment. *S*s in Experiment VI were either shocked, handled, or ignored during infancy (from 21 through 25 days of age). The experimental procedures used for infantile treatment were replications of those used in Experiment IV, except that all animals were weighed at 20, 25, 50, and 85 days of age. In one sense, then, the Ignored group was not truly ignored, although weighing was accomplished without handling by the *E*. (*S* was weighed *in* a removable cage, and the weight of the latter was assumed to be constant within the accuracy of our data.)

Adult Test. At 85 days of age, shocked, handled, and ignored groups all were removed from ad lib. feeding and given access to food for only 1 hour per day. At 100 days of age, *S*s from each group were divided equally and assigned to one of two subexperiments:

1. Experiment VI_A. Beginning on Day 100, *S*s were given one training trial per day on the elevated maze. Reward was 2 grams of wet mash. Following this single training trial, the animal was returned to its living cage and permitted an hour's free feeding. Each day, then, *S* was trained under a 23-hour hunger drive.

2. Experiment VI_B. *S*s were also given one training trial per day on the elevated runway beginning with Day 100, and the same reward was used. However, these animals were not permitted an hour's daily free feeding and thus were exposed to gradual starvation.

The performance measure used in both subexperiments was running time. *S*s were weighed daily during the course of the experiment, immediately before the training trial.

Results

Weight Changes. Table 11 indicates the mean body weight of shocked, handled, and ignored *S*s as a function of age and deprivation schedule. Using analysis of variance, a significant difference between conditions was obtained only at age 25 days. It appears that the shock retarded weight gain during the treatment period, but that compensation occurred during the succeeding 2 months. The mean weight of the Handled group, although slightly larger than the Ignored group at each weighing, never achieved statistical significance. It may be that the trend simply represents the con-

TABLE 11

MEAN WEIGHTS OF GROUPS AT VARIOUS AGES
(Experiment VI)

Age (in days)	Shocked		Handled		Ignored	
20	40.5		40.5		40.3	
25	64.4		71.6		71.5	
50	189.0		198.9		190.9	
85	293.7		298.1		288.4	
	Subexperiment		Subexperiment		Subexperiment	
	A	B	A	B	A	B
100	239.4	238.4	243.8	244.7	238.0	240.4
101	236.2	224.9	241.1	233.1	236.8	228.6
102	232.8	213.7	238.8	221.1	231.5	216.6
103	229.5	204.1	235.1	210.1	229.2	206.3
104	226.3	191.7	231.8	198.5	226.9	195.3
105	223.5	180.6	230.1	186.3	227.0	184.6

tinuation of a small group difference at the beginning of the experiment. These results confirm those reported by Scott (1955) using a much lower shock level, but, with regard to the effects of handling, do not substantially support the data of Weininger (1956), McClelland (1956), and Bernstein (1952). However, it must be pointed out that the handling procedures employed in these latter experiments were much more elaborate and extended over a much longer period of time than the treatment to which our animals were exposed.

It is apparent from the table that both deprivation procedures produce striking and consistent weight losses in all groups. Furthermore, the severe deprivation to which *S*s in Experiment VI_B were subjected accelerated weight loss—and, indeed, caused death in some *S*s by Day 106. Although analysis of variance yielded negative results for the infantile treatment factor, sex and deprivation schedule were highly significant.

Runway Latencies. Experiment VI_A. In Table 12 are presented running times for shocked, handled, and ignored *S*s under 23-hour food deprivation. It is evident that time scores diminish rapidly and consistently for all groups of *S*s as a function of training. The Kruskal-Wallis *H* test, performed on the latency scores for Day 1, indicates that the ignored *S*s are greatly inferior to either handled or shocked groups ($p < .05$). No significant differences were obtained for other training days.

Experiment VI_B. Table 12 shows the time scores for experimental groups under the more severe deprivation procedure. This procedure resulted in death by starvation for all *S*s. The earliest deaths occurred on Day 7 of testing (106 days of age). Thus, latency data are presented only for those trials when the total sample was tested. By means of the Kruskal-Wallis *H* test, it was found that, as in Experiment VI_A, the performance of the Ignored group was significantly inferior to that of shocked or handled *S*s on Day 1. Since the statistically significant inferiority of ignored animals on Day 1 has been obtained in two separate cases, we may reject the null hypothesis with some degree of confidence despite having capitalized on chance by performing individual tests on each training day. The inferiority of the Ignored group once again diminished markedly on further training

TABLE 12

Mean Running Time (in Seconds) for Each Experimental Group as a Function of Training

(Experiment VI)

Trial	Shocked		Handled		Ignored	
	Subexperiment		Subexperiment		Subexperiment	
	A	B	A	B	A	B
1	152.2	148.2	167.5	137.0	263.8	278.4
2	75.1	43.7	63.1	52.3	90.2	67.8
3	45.8	15.1	36.7	19.9	80.3	27.1
4	34.9	10.5	38.3	14.5	44.3	17.6
5	21.8	15.5	30.0	10.7	27.7	10.7
6	16.6	17.8	15.4	8.7	17.4	9.0
7	12.1		9.8		11.9	
8	11.5		10.1		11.4	
9	11.2		10.1		10.3	
10	10.5		9.6		10.1	

trials. By Training Day 4 all groups were performing efficiently. However, on Days 5 and 6 the following occurred: the shocked *S*s began making inferior goal responses. These responses did not appear to be the result of debility; *S*s in all groups responded vigorously on all tests trials until approximately 12 hours before death. The response inferiority of the Shock group produced statistical significance on Day 6 but not on Day 5, using the Kruskal-Wallis *H* test. However, since an individual statistical analysis was performed for each training day, we have capitalized on chance, and a replication of this portion of the experiment is in order.

Age and Weight at Time of Death by Starvation. The frequency of deaths and weight at death in each group was studied. Differences between treatment groups were not statistically significant for either measure.

Conclusions

1. The data on the relative weight of *S*s shocked, handled, or ignored substantially support those reported by Scott (1955): *S*s shocked in infancy gained less weight during the period of treatment than nonshocked *S*s; however, this difference was only transient and cannot be reproduced in adulthood even under conditions of deprivation. Thus, if intense stimulation in infancy produces somatic changes, these changes are not reflected by body weight.

2. Several experimenters (Levine & Otis, 1958; Weininger, 1953) have reported that rats handled early in life show significantly less mortality following food and water deprivation. The data presented in this experiment do not substantiate these findings nor do they suggest that *S*s *shocked* in infancy are more or less viable than controls.

3. Handling was observed to have a beneficial effect upon running times to obtain food at least on Trial 1, but group differences tended to disappear on subsequent trials. Thus, the combined data of this and the preceding experiments indicate that the particular handling procedures employed in this study had relatively little residual effect upon adult behavior. We have already offered an hypothesis to account for our failure to confirm previous results in this area.

4. The latency data of this experiment were quite interesting and need to be replicated. They suggest that nonspecific residua resulting from infantial trauma operate on behavior only under conditions which involve a *substantial amount of stress;* the performance of the Shocked group was impaired only after *S*s had been severely deprived for several days. Coupled with the positive results of Experiment III, where the dependent variable was escape-avoidance behavior under intense shock, and the negative results of Experiment IV, where the dependent variable was exploration, we have placed some limits on the behavioral generality of the consequences of traumatic experience.

Discussion

The foregoing studies provide an overwhelming demonstration of the enduring effects of electric shock trauma. In one sense, of course, such findings are not particularly novel. There is an enormous literature dealing with the effects of shock upon such acts as bar pressing, maze running, exploratory behavior, and shuttlebox jumping. But the present results are distinctive because they involve such a long time between traumatization and testing. To be sure, there have been a number of studies dealing with the retention of CRs over extended periods. The most noteworthy of these undoubtedly are the classical investigations of Liddell on experimental neurosis (see, for example, Liddle, James, & Anderson, 1934, where the 2-year retention of a flexion response was demonstrated); again, Wendt (1937) has shown the retention of an avoidance flexion for 2.5 years.

There is every reason to believe that there are similar, if not identical, mechanisms underlying the short- and long-term effects of shock. However, there is one feature of the present work which distinguishes it from that of, say, Liddell or

Wendt: that is the relative infrequency of trauma and lack of significant resemblance to testing conditions—save for the shock, of course, and certain special training conditions, as in Experiment III; it is rather like the traumatic events which are the subject of clinical speculation and theorizing. Investigations like those of Liddell or Wendt have the clearly structured aspects of conditioning operations in which cue and reinforcement are systematically related. Our own operations involve an undifferentiated traumatic condition wherein the shock and cue factors are related rather unsystematically, and where the long-term changes in behavior are dependent not only upon the nature of the test situation but even appear to be unrelated, in part, to the behavior of *S* during original treatment.

We shall review the major findings and deal with a number of questions and problems which they raise.

Previous Experience and Early Experience

The development of a long lasting effect of shock is, of course, the main "finding." It has been shown in Experiment I that this effect—which we have called a *residual*—is indifferently the same whether it is based upon traumatizing a weanling or an adult. In this sense, the results are only indirectly relevant to problems of development. It is, however, an object lesson for the problem we raised in the introduction, viz., the distinction between *previous* experience and *early* experience. At least with regard to the effect of shock, it appears that the effects of early trauma and previous trauma are the same.

Such a generalization has to be qualified, however, especially as it might be applied to developmental phenomena. Our *S*s were traumatized as weanlings and studied as relatively young adults. It would be dangerous to assume that the effects of shock given prior to weaning, say at 5, 10, or 15 days of age, would be the same as that administered subsequent to weaning. Like most rodents, the rat matures very swiftly. Thus the consequences of severe shock could reasonably be expected to differ even for so short a time as 10 days.[5]

What might these differences be like? There are at least two rather different (but not incompatible) possibilities. The simplest outcome is that there would be no effect; at some period prior to weaning the *S*s may simply be too young to establish a residual with consequences for problem solving behavior; Denenberg (1958) presents evidence on the conditionability of infant rats which supports this. The other possibility is that the effect of shock might be so destructive that the ordinary capacities of the *S*s are permanently changed. It is easy enough to speculate about what these effects might be like; there are ample grounds for expecting permanent central nervous system and endocrine changes. It is more difficult to know in detail what the alterations might be like. Overriding these problems, though, is the question of knowing what are the behavioral correlates of particular damage changes. It is here where evidence is minimal and the need so great.

At the other end of the development picture is the question of senescence. The deterioration of capacities with age is a most complex problem and it is impossible to predict on the basis of gerontological research what to expect about the fate of residua over an interval of, say, 900 days as compared with the period of 100 that we used, where the longer period covers the growth and decline of the organism. It is clearly an investigation that is needed.

Another limitation to keep in mind is that our results may hold only for electric shock. For example, in Experiment V we obtained no indication that exposing weanlings to extreme cold produced any residua. Nevertheless, there is an accumulating body of evidence to show that frequent handling

[5] The work of Denenberg and Levine indicate that for both the rat and the mouse this is very likely the case. In several studies which are now being prepared for publication, R. W. Leary of the University of Oregon has shown that there are, indeed, substantial behavior differences between animals traumatized at 5, 10, and 20 days of age when tested in the runway apparatus used in these studies.

at an early age produces residua which manifest themselves in a variety of ways later in life. On the other hand, the work of Ader (1959) suggests that the age at which the experience occurs may be a crucial factor since he found no differences between animals who were handled beginning with age 23 vs. 136 days. Consequently, there appear to be developmental thresholds for the effects of different sorts of experiences, and it is reasonable to suppose that these differ for different kinds of experience.

So we are able to advance only a rather limited generalization: there is no difference in the effects of a shock over a 100-day interval whether the shock is given at 21 or 121 days of age.

Interval between Trauma and Test

While we have shown that the age at which trauma occurs is irrelevant to the operation of residua over an interval of 100 days, there remains the matter of the length of the interval between trauma and test. We have only one bit of evidence on this question, and this comes from the same experiment on which the preceding discussion was based, Experiment I. While the long-interval animals did not differ from one another they did differ from the short-interval animals; those *S*s who were traumatized at 121 days and tested at 125 did better on escape than the remaining experimental animals and all control groups.

It is difficult to account for this outcome, though there are several possibilities which suggest themselves. First, of course, is the possibility that this is a chance outcome; we hope to answer that by a replication. However, it is our belief that the magnitude of the superiority is too great, exceeding even the control level, to be attributable to chance fluctuation; this is, at any rate, the assumption underlying the remainder of the discussion. A more interesting possibility is that there is some latent feature of the residua which requires a lengthy interval of time to mature; if *S*s are tested after a shorter period, then the usual effect of shock upon escape behavior does not appear. This suggestion suffers from the fact that it does not appear able to account for the superiority of the immediately tested animals relative to control animals; at most it accounts only for superiority over other traumatized animals.

There is a variant of this approach which is more suggestive that stems from the work of Griswold and Gray (1957). They found that electroconvulsive shock reduced the susceptibility of rats to the lethal effects of the Noble tumbling drum (1943); this was in addition to the demonstration that tumbling itself, in small doses, built up almost complete resistance to extended trauma. In our own experiments, it is reasonable to assume that the traumatization operations built up some experiences with shock so that if it were again to be met, as in the alley runway, a relatively organized response to an otherwise disabling experience could emerge quickly. To adopt this hypothesis, it is necessary to assume that the modulating effects of recent experience disappear with time. So long as it lasts its effect is to diminish the drive level induced by shock, a kind of "grin and bear it" phenomenon. If such an interpretation is warranted then the old suggestion that emotional and high drive states reduce the range of available cues that *S*s use (Easterbrook, 1959) can be put to work; the effect of recent traumatization is to adapt the organism in such a way that the drive level induced by shock is lower than for animals traumatized long ago or who have never had any previous shock experiences. The shock experienced under escape conditions does not disorganize recently shocked animals so much, that is, their drive level is not so high, and they are better able to discriminate and use environmental cues leading to safety.

As is true of all ad hoc explanations, the present suggestion has its troubles, the main one being the fact that the immediate animals are not also superior to more distantly traumatized *S*s at avoidance.

Another possibility, suggested by the results of Experiment V, is that the superior performance of *S*s tested for escape behavior immediately after exposure to shock represents the transitory presence of a con-

ditioned running response to the UCS, a running response which is "lost" over a period of 100 days, leaving only our normally observed residue—which, in the runway apparatus, is manifested in longer escape times and superior avoidance behavior. This explanation is satisfactory, however, only if it can be determined that the so-called "normal" residua represent something other than a learned response to shock, since two incompatible, learned responses cannot be used to account for disparate observations stemming from the same antecedent conditions!

In any event, the data suggest that the long- and short-term consequences of shock may be different. Whether this results from the operation of latent or maturing mechanisms or transient conditions cannot now be determined.

Adaptive Consequences of Trauma

One of the notions underlying the interest in the effects of early experience is the possibility that certain experiences may prove to be benign or malignant. This would mean that independently of the kind of problem subsequently confronting the *S*, the effect of having had a certain kind of experience would invariably produce "good" or "poor" behavior. On the face of it, such a notion would not appear to be a good one because the measures of satisfactory performance are so arbitrary for any activity that it is always possible to conceive of measures which place a premium or value on slower, hesitant, or erroneous outcomes. Hence, on purely analytic grounds, an expectation that there will be universally deleterious consequences from an experience, independent of the measures to be used, cannot be vindicated.

The outcomes of the present set of investigations support this logical point. In general we have found that trauma produces inefficient escape behavior—where a lengthy interval between it and test occurs—and efficient avoidance behavior relative to control animals. It is obviously the same set of experiences which does this so there must be something about them which, *depending upon the conditions at the time of testing,* will produce fast or slow, good or poor behavior. The job of understanding the effects of previous or early experience should therefore consist of working out the properties of trauma-induced residua, the properties of the test conditions, and the laws connecting them. Needless to say, the extent to which this can now be done theoretically—as contrasted with inductive generalizations—is sharply limited.

Varying the Treatment Parameters

The question of the specific conditions necessary at time of treatment for the appearance, and for the systematic variation, of long-term, trauma-induced residua has been explored in two ways. First, the shocking schedule has been altered in order to determine whether the length of a single exposure or frequency of exposure is related to the magnitude of the adult changes. Second, by varying the response possibilities at the time of treatment we attempted to learn something about the instrumental nature of the residua.

Varying the Traumatization Schedule. The effects of varying the frequency, strength, or duration of shock upon specific operants or respondents have been explored by a number of investigators. In all cases there is evidence for a relationship, though the exact form is by no means clear. Kimble's (1955) data suggest an inverse relation between response latency and shock strength, whereas Brush (1957) and others have obtained results which suggest that the relationship is nonmonotonic. In these investigations, however, the shock was administered in contingent relationship to a response which had to be learned or had been learned, the data consisting of variations in the rate of acquisition or extinction. The present series of studies involve a shock administered before there is a learning assignment, though it is obvious that the usual responses to shock are clearly related to the running response which we used as a measure. For that reason, indeed, discovering this relationship might be said to be one of our main objectives.

In any case, the data establish a relationship between both shock duration and frequency for the schedules used: Exposure to the apparatus *without* shock (handling condition) cannot ordinarily be detected in adult behavior, and greater amounts of shock per session or greater numbers of sessions increase the magnitude of the adult effect, at least within the parametric limits of this investigation. Notice, however, that the empirical demonstration of this relationship can be used to support a large number of hypotheses concerning the *nature* of shock residua, so that these results may be informative and even supportive, but *not* discriminative in regard to theory. They simply indicate further that, whatever explanation is offered, it must be an explanation which assumes the residua to be affected by some or all of the conditions present when *S* is shocked at 21 days of age, and which allows for variation of shock exposure to affect the magnitude of later behavioral changes.

On the other hand, the *distribution* of shock during treatment does not appear to affect the residua. In Experiment III, *S*s given continuous (E_4) or intermittent shock (E_3) during a single session, and *S*s given short "trials" of shock with removal from the apparatus following each trial (E_2), fared about the same in the adult testing situation. Thus, although the *total amount* of shock per treatment is related to future performance, variations in the way that *S* is exposed to a constant amount of shock (with all other parameters, such as treatment apparatus, controlled) do not appear to be significant. This, of course, argues strongly against an explanation of the performance of these groups based on a simple version of instrumental learning theory.

Providing Ss with Different Behavior or Response Possibilities. The various groups involved in Experiment III give us a reasonably coherent picture of some things which occur during traumatization. One of the most interesting findings fits squarely into the picture as almost any learning theory would see it. The *S*s who were given direct escape practice showed marked improvement in escape running as pups and when tested as adults were superior to all other trauma training groups in both escape and avoidance. While the question of how the escape running is learned may be treated differently by learning theories, there should be agreement about its consequences; all should predict the positive transfer to mature behavior. Insofar as there is such a clear-cut case of positive transfer—more accurately, continued training after a lengthy interruption—there is no problem. Nevertheless, the fact that transfer does occur, indicates clearly that there must be some common cue elements involved; whether these are all environmental stimuli or whether some of them may be based upon shock effects or other endogenous conditions is not clear. One of the cues that *is* clearly involved in the escape practice group that does not hold for other groups, is the raising of the door between the start box and the alley. An increase in running speed requires the animal to begin moving as soon as the door goes up. However, whether there is also independent perceptual learning about shock (for example, discriminative stimuli), is not known.

In any event, the role of direct positive transfer is unequivocally demonstrated. What now about the other groups in Experiment III? For these groups, no provision for learning an instrumental response was made. However, despite marked variations in their weanling treatment schedules, they did not differ as adults. Since they were superior to nonshocked controls in avoidance it is inescapable that the residua are not only the products of instrumental learning such as shown by Group E_1. This is reinforced by the fact that while E_1 was superior in both escape and avoidance, the other shocked animals were superior only in avoidance and were, indeed, inferior in escape.

Further, the behavior of *S*s in Experiments III and V who were exposed to shock while strapped in a harness argues strongly for the independence of at least a part of the residual from cue and response possibilities when trauma occurred. At the time of trauma, these groups could make no overt movement (except of the head) and had no useful visual or tactual cues for behavior in the later testing situations. Yet, in Experi-

ment III, the harness group displayed the typical pattern of inefficient escape behavior, and in Experiment V it showed lowered activity in the presence of shock. Clearly, these behavioral effects do not depend upon similarity of neutral cues between treatment and test, and they do not lend themselves to any explanation based upon the acquisition of an instrumental response.[6]

The only necessary stimulus appears to be the unconditioned stimulus, the electric shock itself. This "pure shock effect" explains the independence of activity and shock-escape behavior for the harness and control groups; furthermore, it explains the absence of a relationship between escape and avoidance behavior in the *same situation* for most experimental groups.

There are, then, two sources of residua so far: direct training (probably involving the visual cue of the escape door being raised) and the shock itself. Are there any other sources? Since the harness groups show the effect of shock only in escape, while other nondirect training groups also show it in avoidance there is every reason to expect another source to be present. What it is emerges from Experiments III, IV, and V.

In Experiment V, *S*s shocked in the black shock box and tested in the closed-field maze having a grid showed the typical "fear" reaction of depressed activity; in contrast, the *S*s in Experiment IV exposed to identical treatment and test conditions *except that the closed field had no grid*, could not be differentiated from controls. It appears, then, that weanlings acquire a fear of the grid bars. Confirmation of such an acquired drive comes from Experiment III, where, again, the black shock box group displayed "superior" avoidance behavior compared with the harness group or controls. That the fear is a drive, in the conventional sense, and not an instrumental response is easily seen from a comparison of the black box groups in Experiments III and V. In Experiment II performance under no-shock (avoidance) conditions is enhanced, whereas in Experiment V performance under no-shock (activity) conditions is depressed.

It appears, then, that we have at least three residua operating in these experiments. To the "instrumental habit" resulting from direct training and the "pure shock" effect exhibited by the harness groups, we must now add a "learned drive" of fear which is conditioned to the grid bars appearing in both trauma and testing devices.

Varying the Test Parameters

Obviously, the previous discussion of variations in cue and response possibilities at the time of treatment necessarily indicated some important points regarding the effects of varying test parameters. It made clear that two of the residua, instrumental habit and learned drive, depend upon the similarity of cues between treatment and test. Thus, marked variations in the test parameters that preclude such similarity, would necessarily destroy the behavioral effects of those two residua. The results of Experiment IV show this to be the case.

The pure shock variable has been constructed from the observations of the harness group of Experiments III and V. In both these experiments it should be noted that modifications of behavior following shock in the harness occur only in the presence of shock at time of testing. Thus, harness *S*s do not differ from control *S*s in avoidance behavior (Experiment III) and they do not differ from control *S*s in activity under no-shock testing conditions (Experiment V). Hence, the effects of the pure shock variable would appear to be observable only in the presence of shock.

That this is not so, however, will be seen from a consideration of Experiment VI. When *S*s were tested on an elevated runway to food under mild deprivation, there were no differences between experimental and control groups. However, when run under "severe" hunger, experimental *S*s ran more slowly than controls *S*s.

[6] The only possibility is that there was a "no response," freezing reaction learned. However, since the response thresholds for the harness groups (Table 10) do not differ from those for the other groups, this possibility must be rejected.

Since treatment and test conditions were the same in Experiments IV and VI except for the level of the hunger drive we must assume that this latter condition is the factor responsible for the different outcomes of these two experiments. Thus it can be seen that to demonstrate the effects of early shock-trauma upon adult behavior, there can be *wide* variations in test apparatus, variations which preclude the use of learned cues. From Experiment VI we may draw the tentative hypothesis that the only requisite testing condition is high drive level. *S*s may be motivated by shock or by hunger, and if it is sufficiently intense, the effects of the "pure shock variable" will be evident.

What Are Trauma Induced Residua?

In the introductory section the term "residua" was proposed as the name for the effects of trauma. This neutral usage was adopted because it was not clear when these investigations were begun what sort of phenomena would emerge. The title of this paper indicates our resolve, even after the analysis of six experiments, to adhere to the neutral usage of the term. The plural form is used to express the conviction that there are a number of factors rather than a single one and to provide room for the possibility that these factors may even be completely independent of one another in their mode of operation.

In an area so little explored yet so rich with allusive terms, theorizing is, of course, dangerous. Nevertheless, in order to guide the discussion, we have introduced three constructs to denote different aspects of the relationship between early shock-trauma and adult behavior. We turn now to a brief consideration of each.

Pure Shock Variable. The pure shock variable was postulated to result simply from exposure to intense shock and to manifest itself in inefficient behavior when *S*s are tested under stress. Thus, the relationships between trauma and later shock-escape behavior, activity under shock, and locomotion toward food under extreme deprivation are accounted for. Contrariwise, activity and locomotion toward food under "unstressful" conditions were not affected by previous trauma.

We are still left with the problem of providing insight into what such a thing as pure shock effect might be. We have far too little information as yet to be confident about any suggestion. Yet, for what it may be worth, we believe that the pure shock effect is closely related to the "activation" theory of Malmo.

In several theoretical articles, Malmo (1957, 1958) has suggested that the "intensity" of behavior may be considered as a separate and theoretically identifiable dimension. There is an extensive experimental literature to support such a view. Intra-individual covariation of EEG, GSR, muscle-potential gradient, heart rate, blood pressure, etc., has been demonstrated under stress and these indices are known to be related to excellence of performance in the form of an inverted U-shaped curve (Bartoshuk, 1955; Stennett, 1957). Furthermore, Malmo and Shagass (1949, 1952) have found that psychiatric patients diagnosed as displaying "pathological anxiety" show higher levels of physiological reaction to stress than nonpatients, so that individual differences in sensitivity to "arousal" perhaps are identifiable with the clinical definition of anxiety.

Malmo has suggested that permanent modifications in relation to stress may be the result of "keeping level of arousal very high over long periods of time." Keeping this in mind, it is possible that the pure shock effect observed in this series of studies represents an experimental demonstration of a change in what has been termed sensitivity to arousal. The pure shock effect, it will be remembered, was observed in every experimental situation used, so long as the organism was at the same time under stressful conditions. The correspondence is obvious, although in this report there is no evidence to substantiate *physiological* changes in intensity corresponding to those reported by Malmo.

Malmo's suggestion that changes in reaction to stress may occur following *extended* periods of high level of arousal was, of course, not tested in our experiments.

Rather, the treatment periods were quite short though involving very intense stimulation. It is possible that the *severity* of the stimulus acted as a substitute for prolonged exposure to a less intense stimulus.

The correspondence of the present results to those dealing with level of arousal is certainly not established, but it represents a particularly interesting possibility, not only because it would be a demonstration of interspecies generalization but also because it would point up the value of attacking a problem from both ends (treatment, as we did; "effect," as Malmo did).

Learned Fear. A drive, conditioned to the grid bars in the shock apparatus, was postulated to account for the efficient avoidance behavior of experimental *S*s in Experiments I, II, and III and the "freezing" behavior displayed on the open-field grid in Experiment V. We believe this drive is the same phenomenon which Miller (1959) has studied so extensively. There seems little reason to doubt now that a fear drive may be conditioned to visual or tactual cues. While it is true that most previous investigations of such learned fears have worked with short-term effects, they should be at least as long-lasting as other learned habits. In view of the particularly great resistance of fear to extinction found by Solomon and Wynn (1953), it seems reasonable to assume that a fear response conditioned to the shock grid bars in infancy may be redintegrated upon being exposed to such bars as an adult.

Instrumental Learning. Under specific and sharply definable treatment conditions an adjustive response of escaping shock may be learned. In Experiment III this learning took place in Group E_1 at a very early age. Again, as in the case of the learned fear drive, there is no reason to doubt that such learning will be retained for long periods of time. Hence, this third residual factor appears to be a reasonable one to postulate.

Comparison with Other Studies

Because of differences in procedures and equipment, the present studies are relatively independent of other work on traumatic or early experience. Our presentation has tended to emphasize the separation. There are, however, two features of work in this area to which we can relate our work; they are shock and handling.

Studies Using Electric Shock as the Critical Variable. Rats which had been shocked while young, when tested as adults by Scott (1955) displayed mild but consistent differences in a number of situations. He also found that weight increases were slowed down in experimental animals during the trauma period. The present studies substantiate this latter finding with the following additional feature: The loss is transitory since animals recover weight by maturity and cannot be distinguished from nonshock controls even when subjected to severe hunger. We could partially confirm his behavioral data; our experimental *S*s also showed greater emotionality, that is, low activity, than controls when the adult test situation possessed features similar to those of the trauma device, but lowered activity was *not* found in a "strange but nonpainful situation." "Nonpainful" is the key term here; experimental *S*s exposed to a strange but *painful* situation as adults reacted much less to the pain than controls.

Levine, Chevalier, and Korchin (1956) used treatments similar to those of Scott in order to study adult avoidance learning. Rats were either shocked, handled, or ignored during infancy (1 to 20 days of age). When all *S*s were tested at 60 days of age for learning efficiency at a hurdle-jumping avoidance task, the *E*s found that the Shocked group was significantly inferior to the Handled group, and that the Ignored group, in turn, was inferior to the Shocked group. In regard to the superior avoidance performance of shocked *S*s, the present set of experiments confirmed the Levine et al. data. But handling early in life apparently had little effect on adult behavior (except for the initial response to food in Experiment VI). Failure to replicate the Levine results for handled *S*s may be attributed to either of two differences in procedure: (*a*) compared to Levine's rats, our *S*s were handled for shorter periods of time—5 or 10 days compared with 20; (*b*)

the *age* at which handling occurred was different in the two studies. Levine's rats were handled prior to weaning; our's were invariably handled following weaning. Therefore, there are no necessary contradictions in the two sets of data, although, together, they suggest limitations in the effectiveness of handling.

Baron, Brookshire, and Littman (1957) have submitted evidence showing that when *S*s traumatized immediately after weaning are exposed later to shock-escape or shock-avoidance test situations where the CR is a lever press, they are consistently superior to controls, making more efficient escape responses and a larger number of avoidances. Now, traumatized *S*s in the present study *did* make more avoidance responses than controls, but *they were grossly inefficient at escaping shock*. These findings appear to contradict those of Baron et al. (1957). Such a contradiction may simply reflect the differences in procedure and equipment. Traumatized *S*s ordinarily run less when they are subsequently shocked; hence, the poorer escape performance in the present series is a logical outgrowth of the lowered reactivity of traumatized *S*s to shock relative to naive *Ss*. Where a lever press is the relevant response, prior exposure to shock should make an animal less susceptible to being disorganized by the recurrence of shock. Hence, it is always important to consider the nature of the behavior being studied, rather than some arbitrary characteristic, such as latency or speed.

Griffiths and Stringer (1952) found that rats shocked in infancy were not different from controls when successively tested on a Warner-Warden maze, a modified Lashley discrimination apparatus, the Hall open-field test, and for susceptibility to sound-induced convulsions. These "negative" findings once again do not contradict other studies, including the present one, where differences were observed. The Griffiths and Stringer experiment utilized a relatively low shock intensity for treatment and confounded the results through successive testing of the same experimental *S*s, a procedural flaw which has been discussed in the introduction to this report.

Finally, Denenberg and Bell (1960) have located critical periods in mice for the effects of infantile shock on adult avoidance learning. Mice shocked prior to weaning differed from controls in avoidance behavior depending upon the age of traumatization and the magnitude of the adult UCS. These data are only in partial accord with those of the present study, for some of the Denenberg and Bell experimental groups were *inferior* to controls. However, their results extend the information on the effects of early traumatic stimulation by suggesting that though the age of the *S* is not a critical variable after weaning, it may very well be a critical variable *prior* to weaning, during the very rapid and distinctive period of maturation in the rodent.

Studies Using "Handling" as the Critical Variable. Recently, a number of studies have indicated a relationship between handling, or gentling, early in life and later viability (Bovard, 1958) and learning and emotionality (Denenberg & Bell, 1960; Levine, et al., 1956; Scott, 1955). These studies have indicated further that handling is an effective independent variable only during a critical period early in the life of the organism. The present set of experiments, although not aimed directly at the problem of handling, supports this position. *S*s handled after weaning in Experiments II, III, IV, V, and VI were not different from nonhandled controls in behavior and viability tests.

Summary and Conclusions

In a series of six experiments, albino rats were exposed to a variety of traumatic and nontraumatic experiences to determine some of the parameters which are critical for the relationship between trauma and later behavior. The over-all results provided a convincing demonstration that trauma in the form of intense electric shock does modify future behavior. More specific findings were:

1. Age of traumatization is not related to its effects, at least if the treatment occurs after weaning (20 days of age). In this

respect the study does not substantiate the general hypothesis of the "critical period."

2. Exposure to extreme cold or handling does not effect *S*s in the same way that electric shock does, at least within the limits of this set of experiments.

3. Coincidental with the behavioral changes created by shock, there are transitory changes in body weight.

4. The "residua" of trauma are probably plural, that is, there is more than one change which may take place in the organism. We have tentatively labeled these changes instrumental habit, acquired fear, and pure shock effect. Each depends on different antecedent conditions and each yields somewhat different behavioral consequences.

5. The consequences of trauma may be adaptive or nonadaptive depending upon the nature of the test situation and are reflections of the three mechanisms we have postulated.

6. Certain modifications of behavior created by prior electric shock are remarkably broad, appearing in all test situations used in this study where drive level is high. Other changes, however, which may be explained by conventional learning theory, are relatively narrow.

Although the results generally support and extend those reported by other experimenters on the relationship between trauma and later behavior, the successive and integrated nature of the six experiments provided an opportunity for more detailed analysis of the critical variables. Tentative hypotheses, based on the results, showed the need for a rather complex theory to explain what trauma does to the organism, but at the same time indicated the need for more empirical work.

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